THE REAL WORK OF SYSTEMATICS[1,2]

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ABSTRACT

Two recent revelations, that the number of species is much greater than previously thought, and that they are disappearing at a frightening rate, should impel systematists to question the implicit objectives of their discipline. It is impossible, using traditional methods, to describe and classify most species in the lesser-known groups. It is suggested that systematists and other organism-ecological biologists must collectively establish criteria for research priorities so that the "real work" of biology in the next few decades can be achieved. Research on many fronts is essential if we are to maintain a significant fraction of the planet's biodiversity.

Blessed is he who has found his work;
let him ask no other blessings.
—Thomas Carlyle, Past & Present,
Bk. III (1843)

WHENCE THE CLASSIFICATION PROJECT NOW?

Two recent revelations should impel a deep analysis of the premises on which systematics has operated. The first revelation is the discovery that 300 years or so of laborious taxonomic work has not brought civilization within 10% of the way to its goal of describing the biological world. This would be arresting even if it were not for the second revelation, which is that the planet is on the verge of an anthropogenic mass extinction that will annihilate much of its biotic diversity even before it can be cataloged.

These facts—the unsuspected vastness of biotic diversity and its current vulnerability—are a major challenge to the taxonomic enterprise, including its goal, its cultural role, and its methodologies. On the other hand, this is an opportunity for systematists to take stock and ask what is the real mission, the "real work" of systematics.

Almost 250 years ago, a great project to classify all life was systematized by the Swedish naturalist Linnaeus, who is considered the father of the modern binomial system of nomenclature. Living systematists are the proud carriers of that tradition. The project has accomplished a staggering amount of work. In all, over 1.4 million species have been described and classified, using time-tested, if inflexible, formulae.

Until recently, it was understood that this project was about half finished. Now we know differently. It is apparent that the biological world is richer than any of us could have imagined just a few decades ago. Recent investigations in the tropics (Erwin, 1988) have increased our estimates of the number of fellow species on this planet from a few million to tens of millions, mostly tropical arthropods. What are the implications of these hordes of undescribed species for systematics? Is this a marvelous challenge or a task of Olympian impossibility? Either way, the great Linnaean project has received a shocking setback—one that must, sooner or later, trigger a searching appraisal by those scientists who see it as their mission to classify and catalog all forms of life.

Of course, the problems are greater for some taxa than for others. For vertebrates, the classification project is certainly more than half done, although ichthyologists are naming some 250 new species of fish each year, most of them small, freshwater species. For a few popular groups—vascular plants, vertebrates except fishes, butterflies—the project may be within 10% of its goal (Peter Raven,

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[2] The title of the paper is borrowed in part from Gary Snyder's book, *The Real Work*. To Joel Cracraft, Paul Ehrlich, Peter Raven, and Warren H. Wagner I am grateful for much good advice, but, obviously, responsibility for these utterances is mine.

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pers. comm.), though formidable challenges remain. Many hundreds of plant species are being described each year. However, the taxonomic task assumes mind-boggling proportions for such taxa as arthropods, polychaetes, and nematodes.

Even before the scale of the recent revelations was apparent, Peter Raven (1977) wrote the following: "It is too late in the history of the world to think that there is time to produce ordered classifications of all plants, animals, fungi, and microorganisms, and then to employ these classifications to seek new kinds of generalities while these organisms are still extant."

Why is it too late? Let's take a quick look at the "extinction scenario," using plants as our example. Plants are numerous, their regional diversity is probably representative of other groups, and they are relatively well known taxonomically. About half of the 250,000 species of plants are associated with just 6–7% of the land surface—tropical forests. Most of these forests are likely to be destroyed or greatly disturbed during the next few decades. What kind of extinction scenario does this portend?

The paucity of information on the geographic extent (range) of most tropical species currently prevents us from estimating accurately the consequences of so much habitat destruction. Most of the current estimates assume that the density of protected areas (nature reserves) is too sparse to capture many locally endemic species. If this premise is wrong, and most tropical species are relatively widespread, extinction rates could be lower than most futurists predict, at least in the short run. A conservative estimate, I believe, would be that 25% of tropical plant species will be extinguished by the year 2020. In other words, we will see a loss of about 34,000 species of plants, 12.5% of the world's flora, within the next few decades. A much larger fraction of insect species is likely to be lost, however, assuming that they are restricted to smaller geographic ranges than plants.

Returning now to the question of the great Linnaean enterprise, do we have the time to finish the classification project of earth's lesser-known taxa (e.g., arthropods, nematodes, mites) before most of them vanish? What would it take to accomplish this task? Recently E. O. Wilson (1988) assumed that there are, at most, about 1,500 systematists competent to deal with tropical taxa. Assuming an average output of new descriptions by this subset of taxonomists of about five per year (this being the historical average, assuming that the effort began a little over 200 years ago), or 7,500 total descriptions per year, and given there are 30 million more tropical species to collect, describe, and classify, it would take 4,000 years to do the job at the current level of effort. (The total number of animal species from all regions described each year is about 10,000.)

Next, let's assume that it is possible to mobilize the necessary economic resources and the educational and scientific talent to increase vastly the number of tropical arthropod taxonomists from 1,500 to a whopping 15,000 "barefoot taxonomists" (Soulé, 1989), thus increasing the output of descriptions of new tropical species from 7,500 to 75,000 per year, or 205 per day. Ignoring the potential information glut and the blizzard of reprint requests, the task would still require 400 years using current procedures. Recall that we have only two or three decades to get the job done, assuming current rates of habitat loss.

So much for this Quixotic brute force option. The training of huge numbers of potentially unemployable systematists is futile, and it cannot begin to meet the extinction challenge. It is also futile to continue the task of describing biological diversity in the manner of the 18th Century, writing, in other words, the slightly premature obituaries of millions of bugs and worms and those of a few thousand randomly selected, undescribed species of plants and vertebrates.

I am not saying that we shouldn't be putting more resources into training taxonomists. Quite to the contrary. For most taxa, there are obviously too few specialists and the need for them is growing rapidly. In addition, our institutions are underfunded; many museums, herbaria, zoos, and botanic gardens have not been able to compete with fashionable "big science" projects, such as mapping the human genome, "star wars" strategic defense technologies, and huge particle accelerators. Finally, we are tantalizingly close to wrapping up several taxa (including plants, vertebrates, some marine phyla, and a few insect groups), and these groups must be completed as soon as possible, so that biogeographic and evolutionary analyses can be based on complete data sets.

Nevertheless, the twin crises of "too many species and too much extinction" will not be solved by the mindless cloning of thousands of taxonomists. Priorities must be established (Raven, 1977; NAS, 1980; Soulé & Kohm, 1989). For systematics and other organismic disciplines, it is not "business as usual" in the closing years of the 20th Century.

Barriers to a Revolution in Systematics

In responding positively to these twin crises, we must look inward as well as outward, confronting

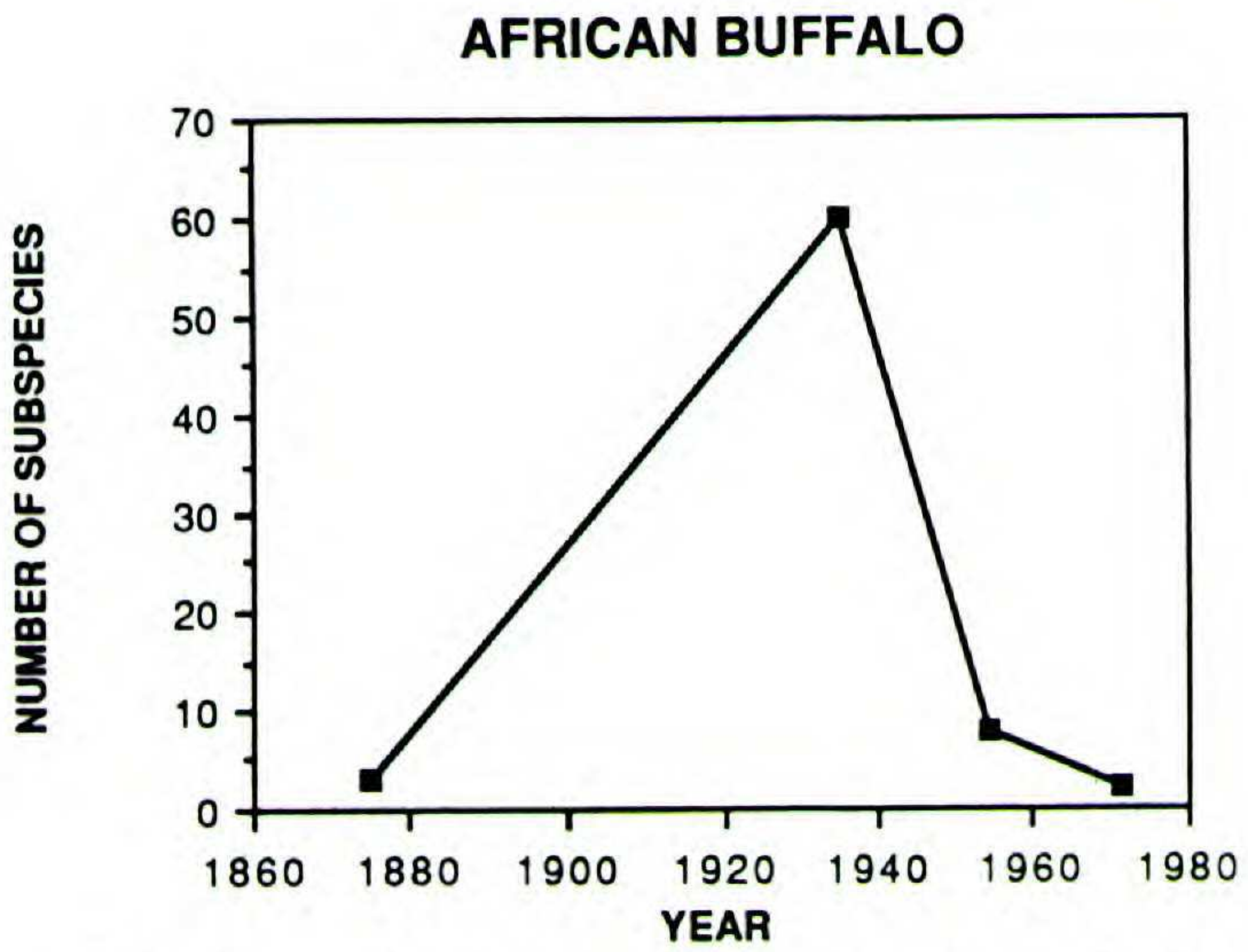

FIGURE 1. An example of excessive splitting for *Bubalus caffer*, the African buffalo. See text.

intrinsic and extrinsic problems. One intrinsic issue is our rhetoric.

EXTINCTION HYPERBOLE?

I am concerned that some of our own rhetoric may come back to haunt us. There is no doubt that humans are initiating a massive spasm of species extinction, and that the rates of extinction may be on the order of 100 species per day in a few decades. But in our panic and despair are we consciously or unconsciously obscuring the complexity of the catastrophe? Is there a conspiracy of silence on the issue of the identity of most of these species?

To biologists, the fact that more than 95% of these threatened species are arthropods and nematodes hardly makes the problem less serious, but to the man on the street, this bit of information would change matters considerably. Such little critters don't arouse a lot of public concern—just the opposite. Most people don't consider arthropods and helminths to be animals. Most people, if they knew this truth about the upcoming extinction spasm, would probably say "good riddance."

Among the more popular taxa, plants and vertebrates, the losses will not be in the millions nor will they come close to approaching a rate of extinction of 100/day. The point that we need to make is that there aren't millions of kinds of birds and mammals. In fact, there are precious few. Relative to the vast number of beetle and mite species, there is hardly a handful of these vertebrate relatives.

On the other hand, we need to explain that: (1) you don't have to have warm blood to be an animal, (2) these "creepy" species have the same right to existence as their green and charismatic cousins, and (3) the demise of vast numbers of bugs and worms is signaling the loss of habitat and ecosystem services (Ehrlich & Ehrlich, 1981) on which all other species, including humans, depend.

ANACHRONISMS AND CHALLENGES

The closet of systematics has more than its share of skeletons. For example, many systematists have pointed out that taxonomy fell into disrepute in the early decades of this century because an extreme form of typology prevailed that led to the application of species and subspecies names to the most trivial intraspecific variants. (The field is still plagued by pockets of extremists.) As shown in Figure 1, for example, some people apparently got a little carried away in describing "new" African buffalo. The zenith of "splitting" was reached in about 1935 when Zukowsky on one occasion gave different, new racial names to the two horns on a single African buffalo *Bubalus caffer* (Ansell, 1971).

We may smile at the excesses of the past, but these excesses are symptomatic of a perennial debate—the species problem. Should every geographic (evolutionary) entity be elevated to species rank, as Cracraft (1983) suggested? Some conservation issues would be instantly solved by such a solution, at least in theory. For example, if subspecies no longer existed, there might be less debate about whether they should be interbred in propagation projects. Of course, it is a delusion to think that this would really solve the problem, because genetic relatedness would still be a matter of degree. Fortunately, many zoos have already adopted genetic rather than strictly taxonomic criteria for such matters (Benirschke et al., 1980; Ryder, 1986; Ryder et al., 1988).

Practices in taxonomy are changing, but are they changing fast enough (Ehrlich, 1964; NAS, 1980)? Methodological stability in such a quasi-legalistic field is commendable, but there must also be sufficient flexibility to accommodate changes in technology and other conditions. For example, the methods for storing museum specimens (most specimens are still preserved as dried organs or as bleached cadavers—in biochemical terms, degraded protein and decomposed DNA) must take into account advances in technology as well as the unfortunate fact that many of today's specimens will become proxies for tomorrow's extinct taxa. Of what value will these specimens be to a 22nd-Century biologist? Put another way, how much more valuable would they be if they were biochemically intact? This is not to say that specimens preserved in the traditional ways (drying, fumigating, denaturing) lack scientific value. DNA fragments can be recovered from specimens that have been in alcohol for decades (Pääbe, 1985), but this bit of serendipity should not be used as an excuse to decelerate the rate of conversion in many museums to more modern methods of preservation. These days, a collector without dry ice or liquid nitrogen is an anachronism.

"THERE ISN'T TIME FOR MORE RESEARCH"

Extreme anxiety can also be a barrier to change. If the field of systematics is to live up to its potential in the campaign to preserve the diversity of life forms, then it cannot fall victim to slogans born of ignorance. To be specific, one often hears that we already know enough—that we have enough knowledge about the systematics, biogeography, natural history, ecology, and genetics of organisms and ecosystems to establish a rational and comprehensive set of nature preserves that would protect most species from the coming Armageddon. It is asserted that there isn't time to indulge in research—we must simply buy more land and lock it up. Is this true? Do we already know enough to protect biological diversity?

We don't. Research in conservation biology during the last 15 years has altered fundamentally the design criteria and management objectives for protected areas. The rising curve of new, management-relevant discoveries shows no sign of asymptotic approach. Why do I raise this issue here? It is not to point the finger at systematists, who are rarely the perpetrators. Rather it is to enlist the support of systematists for conservation biology *sensu lato*.

Many kinds of knowledge are necessary for the successful design and management of protected areas and propagation projects. The first step in many situations is to describe, inventory, and map biotic diversity. This is because the exact locations of reserves are critical, particularly if the objective is to protect the maximum number of species. The entire conservation enterprise depends on systematists and biogeographers for guidance about where to place reserves, particularly in regions where there exist "hot spots" of endemism and species diversity (Diamond, 1986; Gentry, 1986; Myers, 1988; Soulé & Kohm, 1989).

On the other hand, the *maintenance* of biodiversity in a fragmented landscape is a more complex matter. It depends on scientific progress along many fronts, among which systematics is just one. Even if a reserve is in the right place, it will gradually lose many of its species unless managers are attuned to the effects of climatic change, fire regimes, siltation and sediment load (in marine and aquatic systems), patch dynamics, sea level rise, pollution, edge effects, viability of keystone species, loss of mutualists, migratory life cycles, and current and potential human interactions with species and wildlands. Ignorance of these and other phenomena will produce a bitter harvest of conservation failures and wasted resources. It goes without saying that most of the above kinds of research depend on a taxonomic foundation (Boom, 1988).

Ecological research in the last decade has led to profound changes in the ways that conservation projects are designed and managed. For example, only recently have we realized the ubiquity of the deleterious effects of edges. The effective size of a reserve is often much smaller than its map size would indicate because many diversity diminishing agents penetrate great distances into reserves. Many of these edge effects are only beginning to be understood. For example, Appanah (1987) pointed out that meliponid bees nesting as far as 1 km from the edge of a reserve were returning to their nests with 100% pollen from plantations. Because these bees are important tree pollinators in the forests of south Asia, such behavior could lead to widespread reproductive failure and the gradual die-off of forest interior species.

The future status of the Everglades National Park at the southernmost tip of Florida, one of the richest landscapes in North America, offers an example of how recent discoveries can and should modify our management methods and priorities. Larry Harris (pers. comm.) has pointed out that the gradient in this part of Florida is 1:25,000, and that, given the accelerating rates of sea level rise (Titus, 1986), there is virtually nothing we can do to prevent the disappearance of most of this park under Florida Bay and the Gulf of Mexico in 50 to 100 years. There are many endangered species in the Everglades, but existing recovery plans have ignored this inevitable source of habitat loss and its implications for population viability.

Genetics provides many examples of how very recent studies have altered the management of conservation projects. Genetics was virtually ignored by managers until the late 1970s. A few prophets (e.g., Frankel, 1974; Seal, 1978) had earlier warned of the hazards of inbreeding and the loss of genetic variability, but there were hardly any data from rare or captive species, and there were certainly no concrete guidelines of use to managers. Only in the last 10 years has evidence of widespread inbreeding depression in captive groups been uncovered (Ralls et al., 1988, and references therein). It is only in this decade that guidelines and protocols for wild and captive stocks have been suggested (Franklin, 1980; Soulé, 1980;

Frankel & Soulé, 1981; Schonewald-Cox et al., 1983; Templeton & Reed, 1984; Lande & Barrowclough, 1987). Notwithstanding that the original caveats accompanying these principles and guidelines have been largely ignored, it is impressive how rapidly genetics has been assimilated into the mainstream of captive breeding, recovery planning for endangered species, and the management of small groups of large animals.

The problem is that once assimilated, people tend to take such information for granted, and to forget how important conservation biology has been in shaping current management practices. During crises, the value of past research is often ignored. In doing this, we commit an even graver and more perilous sin: discounting the value of future research.

Those who believe that we cannot simultaneously secure land *and* do more research fail to appreciate that, metaphorically, the storage of a precious book or painting in a secure vault does nothing to prevent its gradual, chemical deterioration. To put aside land without knowing how to manage it is folly. It should be noted parenthetically that agencies like the National Science Foundation (NSF) may be legally barred from purchasing lands in developing countries, but they are not barred from funding research that would help insure the protection of biotic diversity on such lands.

In our panic to secure the few remaining bits of wild nature, we should not forget that our understanding of biological diversity, particularly in the tropics, is shockingly superficial. Therefore, we have no choice but to proceed urgently to study the basic mechanisms that fuel, threaten, and maintain the biotic complexity of this planet. Given the rate of habitat destruction, much of this research must be accomplished within the next few years or decades at most. The maintenance of biotic diversity, in situ and ex situ, will depend largely on the quality and quantity of these studies.

The next step is to ascertain the most critical research needs. Recently, a workshop sponsored by NSF was convened by the Society for Conservation Biology (SCB) at Hawk's Cay in Florida to frame a report on research priorities in conservation biology (Soulé & Kohm, 1989). Following are the most pressing and important initiatives and research needs agreed upon at the workshop. Systematics is at the heart of the first priority.

1. *A crash program to carry out extensive surveys and mapping to identify areas that are critical for the protection of nature and genetic resources.* Reiterating the recommendations of an earlier report (NAS, 1980), these critical areas should be defined in terms, among others, of their high biotic diversity, high levels of endemism, or imminent destruction of critical or unusual habitats and/or biotas. These studies should emphasize taxonomic groups that are better known or those that would indicate parallel biogeographic patterns in groups less amenable to censusing. A byproduct of this research could be critical information on the rates of deforestation and other forms of habitat destruction.

2. *It is particularly important to understand how natural systems "work," especially in the tropics.* Therefore, the group called for the immediate establishment of a small number (perhaps four to eight) of research sites in the tropics in order to carry out a coordinated program of comparative research on populations, communities, and ecosystems in relatively pristine and secure situations. The workshop participants agreed with the authors of the authoritative NAS report, *Research Priorities in Tropical Biology* (1980), in recommending the establishment of several major ecological research sites in the humid tropics where in-depth, long-term, and globally coordinated studies are supported.

These focal sites would be especially valuable as sources of long-term, baseline information on global and ecological processes. The SCB/NSF workshop also recommended the active participation of local students, professionals, and institutions in this program and other research projects in their developing countries. One reason that the group did not recommend a large number of such sites is because there are too few researchers with the necessary expertise.

3. *Studies at all spatial scales to assess the kinds, mechanisms, magnitudes, and impacts of humans on ecological systems.* Here are included the effects of habitat fragmentation, biotic mixing (introductions), and air, water, and marine pollution. These studies should focus on the development and evaluation of alternative means of exploitation and land/water use, with the goal of improving human welfare while minimizing environmental deterioration and the destruction of biological diversity.

4. *Studies on the physiology, reproduction, behavior, ecological interactions (including diseases), and viability of individuals, populations, and species have been essential in the protection and management of reserves and other wildlands.* The group urged the enhanced support

for research that focuses on these fundamental, species-level processes and relations, especially with regard to species of critical ecological or economic importance.

5. *Education in conservation biology, wildlands management, and related areas with the objectives of training basic scientists and natural resource managers, particularly in tropical, developing countries.* Much of this training should occur locally and regionally, and should benefit local institutions and strengthen the conservation and management infrastructures in developing nations.

The above list omits direct mention of global phenomena that affect landscape arrangement and habitat quality. These phenomena are of paramount importance for the protection of biodiversity, and are being intensely studied by other groups of experts.

To summarize, the maintenance of biotic diversity in protected areas and in ex situ facilities will depend on many disciplines. Success will require a level of tolerance and a degree of scientific pluralism that are uncharacteristic of organismic and environmental biologists. Systematists should take the lead in this "new age of biological brotherhood."

What Roles for Systematists in the Biodiversity Crisis?

Granting the central role of systematics in conservation biology, what can be done to involve more systematists (and other organismic biologists and ecologists) in the campaign for the protection of biological diversity? First, let me restate the problem: there are far too many taxa to be named and classified in the short time remaining (Ehrlich, 1964). Systematists might consider the following suggestions, none of which are original with me.

1. *For relatively unknown groups like tropical insects, concentrate on a few "representative" taxa*, hoping that the phyletic, morphological, biogeographic, and ecological patterns manifested by these taxa are typical of related ones. Which groups should be chosen? Who would choose them? How should interim conclusions be validated? Answers to these questions require an unprecedented level of cooperation, compromise, and organization.

2. *Focus on ecologically keystone and indicator taxa and their mutualists.* In other words, adopt an ecological approach, letting nature's structure help shape taxonomic priorities, in contrast to the present haphazard selection process driven by economics (agriculture and entomology), personal whims ("I *like* sceloporine lizards"), and theoretical issues in evolutionary biology.

One such approach is to concentrate on taxa containing keystone species. Within the last decade or so, conservation biologists have emphasized the critical ecological roles of keystone species. Defined operationally, a keystone species is one that, by its effective absence from a system, results in the virtual disappearance, directly or indirectly, of several other species, causing, in other words, an extinction cascade. "Several" is undefined, and further work on the utility of this concept is obviously necessary.

In addition, the concept is in need of a great deal more empirical and theoretical analysis. Several avenues are being pursued (Mills & Soulé, in prep.). For example, one can divide keystone species into two categories, *trophic keystones* and *structural keystones*. The absence of the former kind will lead to dramatic changes at one, two, or more trophic levels below. The absence of structural keystones leads to changes in habitat, which in turn causes significant shifts in abundance of other species that may or may not be trophically connected to the keystone. Table 1 lists some of the categories of keystone species and gives examples of the likely consequences of their effective loss (including human-induced rarity) from a community.

Effective management of protected areas depends on an understanding of the interactions of keystone species. Terborgh (1986) found, for example, that figs and palm nuts are keystones in a Peruvian tropical forest, where they may be the only food resources during the annual period of food scarcity. Palm nuts escape from all but a small group of specialist species that are either large and with powerful jaws or else can gnaw the nuts. Thus, among the few customers are peccaries and capuchins, comprising ca. 30% of the total biomass of fruit-eating animals. Fig trees, though they fruit irregularly, are often keystone producers; figs are heavily consumed by all larger primates, procyonids, marsupials, and many birds. Terborgh (1986) concluded that a group of only 12 plant species (out of 2,000) maintains almost all large frugivores for about three months of the year.

If we are to succeed in the protection and management of the remnants of biodiversity, there is an urgent need to refine and deepen our understanding of keystone species, especially in the tropics where most of the planet's biological riches exist

TABLE 1. Some kinds of keystone species and the effects of their effective removal from a system.

Keystone category	Effect of removal	Examples
Trophic/resource keystones		
Top predators	Large increases in the abundances of prey species and smaller predators, and subsequent extirpations of some of the latter's prey species	Felids, canids, fishes
Pollinators and other mutualists	Failure of reproduction and recruitment in certain plants; disease and dieback of plants, bleaching in reef-building corals, etc.	Hymenoptera, Lepidoptera, symbiotic algae, fungi
Providers of essential resources	Local extirpation of dependent animals, including fruit- and nectar-eating species during times of scarcity	Trees, such as *Ficus* and trees that provide nesting and hibernating sites
Structural keystones		
Species that maintain landscape features	Disappearance of water holes and wallows, ponds, etc.	Tapirs, beavers, alligators
Herbivores that prevent succession	Return of cover and decrease in habitat diversity; disappearance of species dependent on early successional habitats and resources	Moose, elephants, rabbits

and are currently at great risk. Current research efforts in this regard are pitifully inadequate, and the low level of funding for basic research in tropical ecology in general is simply scandalous.

3. *Focus systematic work on phylogenetic relicts or other evolutionary outliers.* Evolutionary outliers contain precious information in their genotypes and phenotypes and should rank at the top for systematic attention. Obvious target taxa include oligotypic marine phyla, proboscidians and other remnants of the great mammal radiation, and the last representatives of genera and families that are about to disappear, such as *Hibiscadelphus* in the Hawaiian Islands (Gentry, 1986, and references therein).

4. *Focus systematic work on local endemic taxa inhabiting vulnerable environments.* Habitat fragmentation and regional and global climate changes will eliminate local endemics, including those that inhabit estuaries, reefs, boreal mountain tops in low latitudes, and tropical forests. Other kinds of biogeographic cul-de-sacs harbor some interesting examples. A case in point is the fauna that is restricted to the relatively cool waters of the north end of the Gulf of California, Mexico (Brownell, 1986; Perrin, 1988). As global warming develops, many of these taxa, including the porpoise *Phocoena sinus*, are likely to be squeezed out of existence, trapped between the land on the west, north, and east, and the warm waters to the south.

5. *Develop new (interim) approaches for describing and classifying*, especially for groups like tropical arthropods for which traditional approaches are too slow by orders of magnitude and for which binomials can wait (Soulé, 1989). I'm not speaking of DNA fingerprinting using restriction fragment length polymorphisms, nor the use of mitochondrial DNA, nor the use of electrophoresis for the detection of protein variants and the measurement of their frequencies, nor DNA hybridization studies, nor similar techniques. These are all too costly and time-consuming. I am referring to automated methods for screening, cheaply and efficiently, very large numbers of specimens and assigning them to taxa of convenience. These methods might include three-dimensional tomography for computerized clustering based on morphology and automated chromatographic screening for clustering based on biochemical patterns in such tissues as cambium and hemolymph.

Other suggestions have been made that would enhance data retrieval, comparability, and depth. For example, data entry formats should be developed by international teams of systematists and conservation biologists (ecologists, geneticists, and biogeographers) to ensure that the data will be useful to those asking different sorts of questions. Unless such a team approach is implemented, and such formats and methods are standardized by international agreement, much of the current investment in data banks for systematics will be wasted.

Education is also a high priority. Systematists are in a better position than most biologists to plant the seeds of a conservation ethic. They are often in a position to train local people and to engage leaders in discussions about the latter's biotic patrimony. As teachers, systematists can profess the love of nature to students, the younger the better.

On Our Real Work

The last of the preceding proposals brings me to my final point—it is OK for systematists to speak of love and beauty. It is even OK for systematists to express emotions in public. Real adults are not afraid of labels like "emotional" and "sentimental." Real adults have left behind the Rambo developmental stage and its preoccupation with *machismo*. I don't mean to deny or even to denigrate the harder aspects of human nature. The "right stuff" is adaptive in many circumstances, and ambition can be harnessed just as well for the protection of creatures as for their destruction. Nevertheless, I believe that maturity includes the courage to embrace publicly stewardship as a "familial" responsibility. Giving succor to the earth is our final and most adult task, our real work (Snyder, 1980).

Some may wonder how can we be effective, let alone charismatic, in communicating these feelings of kinship and concern in our work (Wilson, 1984; Soulé, 1988)—our research and teaching—without appearing foolish. Perhaps, we can't. Perhaps, for a real adult, appearing foolish to less mature peers is as inevitable as work itself.

By "work" I am referring to our careers as well as to our real work, which is to love the earth by preserving its actual and potential diversity. Regarding career, Freud said some interesting things about work and its relationship to happiness. For him *Liebe und Arbeiten*, love and work, were not separate compartments. Freud considered the professional work of "civilized" men and women, with all its grasping for recognition and respect, as sublimation for the giving and receiving of love. If there is some truth in this, if each of us wishes to contribute something of lasting value to the world (giving love), and to be acknowledged for it (receiving love), then we biologists are fortunate indeed. It is relatively easy for us to express our love of biotic diversity through our research, writing, mentoring, and teaching. And in return, there is not only peace of mind, but also the gentle fellowship of coconspirators. For us, conservation biology *is* the synthesis of love and work.

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